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THE IMPACT OF THE HIGH DEVELOPMENT COST OF ADVANCED FLIGHT PROPULSION SYSTEMS ON DEVELOPMENT POLICY

B. Pinkel

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The RAND Corporation
SANTA MONICA • CALIFORNIA

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PREFACE

This Memorandum is a continuation of RAND's study of research and development management. It addresses the area of novel and advanced propulsion systems characterized by high estimated development costs, deficiency in technological data, and usually an absence of an application requirement. It discusses management policies for reducing risk when the capability and utility of proposed engines are uncertain.

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SUMMARY AND CONCLUSIONS

Major proposals for engine development now fall largely into two categories: (1) advanced conventional engines with performance specifications closely coupled to discrete missions, and (2) novel engines of a kind not previously developed and for which crucial design data are lacking. These major proposed developments are in general very expensive, and there is, in many cases, no associated approved mission requirement.

This Memorandum discusses how R&D funds should be managed to reduce investment risk when the capabilities and utility of proposed engines are uncertain. With respect to funding management, R&D activities are described in detail to emphasize (a) the design-data-acquisition process that occurs during research and the initial developmental phase, and (b) the extensive ad hoc performance tailoring and defect elimination narrowly related to specific hardware that characterize much of the later stages of the developmental process.

A broad program at the frontiers of science to reveal and apply new methods of obtaining thrust and power is in general advisable; however, the effort in each case should initially be focused on the crucial primary problems, and only after some success should the program be enlarged to include ancillary problems.

Many more systems will be investigated at the research and exploratory-engineering level than will be found worthy of advancement into the development phase, hence a review point should be definitely programmed to decide on whether the effort should be curtailed or advanced before a major investment is risked. Furthermore, plans for establishing expensive laboratories should be based on a broader program than just the support of an advanced engine of uncertain merit.

Cost-effectiveness and mission studies concerning a proposed novel engine, although necessarily rough and in need of judicious interpretation, should nevertheless be made early to guide establishment of the scale and scope of the associated R&D effort.

When the application prospects of a novel engine are uncertain, major emphasis should be placed on acquiring properly documented

design information. On the other hand, the extensive and costly tailoring and fixing that characterize the process of obtaining a developed article should be avoided.

Engine R&D is, of course, too complex to be completely adjudicated by a few simple rules. Good judgment may favor the full development of an engine, particularly when it promises a unique performance capability, even if official approval for the associated mission cannot be obtained; however, the following criteria should be met before the engine qualifies for consideration:

1. The engine promises major improvement in future missions of credible merit.
2. The required design information is on hand.
3. A practical initial application has been analyzed and a set of engine performance requirements is available for orienting the objectives of the initial development.

In the development of an advanced engine containing critical problematical components (i.e., components on which essential design information is lacking), the initial R&D effort should be focused on these components, with major emphasis on systematic and complete data acquisition. And only after some established performance goals have been attained on the problematical components should the program be enlarged to encompass the complete engine and to advance into the developmental phase.

In the absence of an adequate developmental program, the early scheduling of flight-test programs poses an unwarranted investment risk. There is a high probability that flight tests, even if successful, will provide little design information and that the usual extensive ground-test program will be required in any event.

The history of the Aircraft Nuclear Propulsion System is reviewed for the instructional value that it might have for future R&D planning. The advisability of the following was indicated:

1. Avoid large investment risks in concurrent development of well-understood but expensive items (e.g., the turbojet engine in the ANP project) while uncertainty attends the capability of the novel crucial problematical component (e.g., the ANP reactor).

2. Consider in the early R&D planning on a novel engine with an estimated development time of more than a decade that major changes may take place in the mission-application concepts for this engine during this period. (Thus early commitment to engine performance requirements that increase investment risk may not be advisable.)

ACKNOWLEDGMENTS

The very helpful comments and suggestions of T. K. Glennan, Jr., and R. L. Perry are gratefully acknowledged.

CONTENTS

I. INTRODUCTION

In the period through World War II and up to about the mid-1950's, the major propulsion-system developmental effort was applied to reciprocating engines and then turbine engines. The cost of development of the initial engine of each type was small, and there was an obvious continuing requirement for more powerful and better engines. The basic scientific phenomena were in the main well understood, and there was a growing engineering background in materials, structures, cooling, combustion, and fluid-flow information to permit a continuous improvement in performance parameters. The contracting agency, striving continually to improve weapon systems, usually asked in each new contract for an advance in engine performance. The contractors likewise in their competitive zeal tried to push performance parameters, like specific weight and specific fuel consumption, as far beyond the prior values as they dared.

The uncertainty in the success of a development of these engines came from the possible fallibility of the designers in this game of "technological brinksmanship." Did they use good judgment in selecting which components to favor in pushing into the regions of uncertainty in strength? Did they have the expertise to shape components accurately so that the desired increment in flow or combustion efficiency was attained? Did they know enough about improved materials to increase combustion temperatures the promised amount over the going values?

If the designers chose wisely, then only a small number of failures occurred on the test stand. On the other hand, in an overly venturesome design so many failures occurred on the test stand that the program funds and time were expended before a successful engine was attained. The contracting agency usually stopped the development when it appeared that overruns in time and funds were not justified by the application benefits expected from the engine or that greater success was being obtained by a competing contractor.

In addition to good judgment there was also an element of chance in the success of a new design. Strength margins were usually so small

that additional stresses introduced by resonant mechanical vibrations and thermal cycling, which were often very difficult to anticipate, might cause failures that could bring a project to the point of cancellation.

Thus, nearly every new engine in a series of an established type (like the turbine engines) was an adventure into design uncertainty, and this was reflected in development time and cost. In spite of the benefit of the accrued learning to later models, development costs did not decrease with time but rather tended to increase because of the upward trend in size, complexity, and performance goals. Because of the pressure to meet an early operational date for the system, there was a tendency to make early decisions on design and to follow a policy of concurrency which involved simultaneous development of all the components of an operational propulsion system. In some cases even some production facilities were procured during development to expedite fabrication of the many test units required for a crash program, and airframes were committed to still undeveloped engines. This development policy, of course, involved the risk that design changes might be very expensive because they might impose changes in associated systems and even production tooling. Parallel engine developments were often initiated with several contractors to counter the uncertainty in contractor capability.

In this period the contest between offense and defense, i.e., bombers versus fighters and antiaircraft guns, led to a continuing requirement for larger, faster, and higher-flying aircraft and generated a nearly predictable stream of engine requirements. It provided some flexibility in aircraft development planning in that one could usually count on a follow-on higher-thrust engine if the engine developed specifically for a given airplane proved to be inadequate.

The advent of the ballistic missile into our military program in the mid-1950's put an end to this era. The position of the manned bomber as a weapon-delivery system was undermined by the ICBM to the point that a successful case has not been made to date for any bomber beyond the B-52 and B-58. The defense missile has forced the bomber to abandon the higher-faster formula and seek invulnerability in other

modes of attack like sea-level penetration. Thus the large effort on a stream of fully developed turbojet engines of increasing thrust has dwindled away, and there is now interest in developments aimed at very special applications like vertical-takeoff aircraft, the large logistic carrier, the sea-level recce-strike airplane, and the supersonic transport. This greater specialization has forced a closer tailoring of engine requirements to the mission application.*

The impact of the military requirement for ballistic missiles and of the national space-program requirement for boosters caused a shift in developmental emphasis to the chemical rocket. The contest between offense and defense has now taken a new turn. The counter to the anti-ballistic missile may be in more sophisticated ICBM payloads. Although there is some upgrading associated with the established ICBM force, there is not the continuous stream of engines being generated by the offense-versus-defense duel that characterized the period of aircraft dominance.

The space program with its continuing demand for increased payload to orbit did generate a more predictable family of engines of increasing thrust, e.g., the LOX-JP engines (E-1, H-1, F-1) and the LOX-H_2 engines (RL-10, J-2, M-1). These engines range up to about 1.5 million pounds of thrust, which satisfies the needs of the Apollo program. Except for the M-1, which is having funding problems because it has no programmed application, these engines are well along in their development. The prospect for higher-thrust engines explored in the Nova and Post-Nova studies depends on whether or not manned exploration of space becomes an approved item in the space program as a follow-on to Apollo, and this raises a basic question that faces current engine proposals in general: What level of effort is justified on an expensive engine development when the application is uncertain?

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*The engine contractors are attempting to advance their technology by exploratory development of a gas generator comprising a compressor, combustor, and turbine, which, with the addition of fan, compressor, and turbine stages in various combinations, may be used for a variety of possible missions. In this manner they hope to enhance their readiness while uncertainty attends mission selection.

There is also a continuing effort to improve chemical rocket systems in performance and reliability through improvements in propellants, materials, discharge nozzles, cooling, and thrust-vector control, high combustion pressures, etc. The high cost of placing payload into orbit has evoked study of recoverable-booster concepts in anticipation of a large space-transport operation. A substantial number of small special-purpose engines are being developed for a number of tactical missions and for special space operations like attitude control, rendezvous, and lunar landing and takeoff. The development costs of the small engines are sufficiently low that the tying of an engine development to an approved mission application does not become a critical issue.

Starting at about 1950 and continuing into the present era, a large number of very novel* propulsion systems have been proposed: these include (a) systems based on the nuclear reactor, like the nuclear turbojet (ANP),** nuclear ramjet (PLUTO), nuclear rocket (ROVER), the gas-core rocket, and the nuclear electrical system (e.g., SNAP-50 applications); (b) some exotic air-breathing systems like the supersonic combustion ramjet (SCRAM) and the air-collect systems (ACES); (c) continuous nuclear-fusion systems; and (d) the pulsed nuclear rocket (ORION).

Interest in these systems stems from the belief, based on preliminary estimates, that they promise

1. A new domain of flight operation unattainable by established systems; e.g., a large step advancement in flight speed, range, endurance, or altitude, or a new type of mission.

2. A substantial advantage over established systems in similar missions; e.g., greater payload or lower missions cost.

The principal uncertainties attending a novel engine are

1. Can useful performance be obtained and can materials withstand the operational environment?

2. Is there an application for this performance capability of sufficient worth to the nation to warrant the investment in this system?

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*The term "novel" is used in the present discussion to designate a system of which the first of its kind has yet to be developed.

**Aircraft nuclear propulsion.

It has been demonstrated that a new propulsive capability often generates new applications. One would expect this to occur more with the development of novel engines than with the improvement of conventional engines, the applications of the latter having been extensively explored both in practice and in studies of advanced systems.

If development of a novel engine is inexpensive, then it might be justified by the prospect that a new flight capability may eventually find application. However, these novel engines usually entail very expensive developments, and the question of application becomes crucial. Arguments often heard in defense of an extremely expensive development proposal are that (a) it is essential to demonstrate the advanced flight capability of a novel system as a proof of feasibility and utility; (b) the existence of an engine with a new capability will generate new application concepts; (c) the novel engine has a long development time, and if one waits for an approved application, then attainment of an operational system may come too late to be effective; and (d) the present insistence on a mission requirement to support a very expensive proposal would completely block the development of novel engines.

However, the national budget for engine development cannot stand the unrestricted application of this adventuresome developmental policy for the novel engines. The high cost and high risk of these developments are exemplified by the ANP and PLUTO projects, which were canceled after investment of $1 billion and $200 million, respectively. The nuclear rocket without a firm mission requirement was approved for full development at a cost now estimated at $1.5 billion. The developments of other proposed novel engines previously listed are likewise estimated to cost in the multibillion-dollar range. The nation cannot risk many developments of this cost, nor, on the other hand, can it ignore the possibility of major breakthroughs in flight capability from advanced propulsion systems. Thus much more careful planning of a program on novel engines is required that will obtain for the allotted budget the maximum in information and developmental products.

The following conclusions on developmental policy are derived from RAND studies on conventional engines (turbine engines and chemical rockets):[1-8]

1. A strong program at the research level to provide design data for advanced systems is a good investment. Research, which is relatively low in cost, provides a sound technological basis for advanced systems and might prevent some expensive mistakes in development.

2. Several alternative versions of problematical components should be subjected to test prior to a major commitment of funds to development of a specific system.

3. To the extent that it is feasible, engine development should be independent of weapon systems in the early program phases to avoid premature and costly involvement in requirements and ancillary systems that may not be needed in the ultimate application.

4. Uncertainty in the attainment of an engine for an important application can be reduced by developing in parallel several alternative systems of different designs.

We are at a point in the development of turbine and rocket engines where the continuing demand for ever more powerful engines with predictable requirements has largely abated. The engines being proposed are more closely tailored to specific applications; some are extremely costly to develop and lack a firm application requirement. The novel engines likewise have very high estimated development costs and usually no firm mission requirement. Although much of the policy derived from past conventional engine experience still applies, a more detailed consideration of the developmental process is now required for more careful management of funds and effort. The novel engines are supported by much less scientific and engineering information than the advanced conventional engines, and this affects the choice of starting point in the development cycle and the initial scale and scope of effort.

This Memorandum attempts to indicate how unnecessary investment risk in development of advanced engines might be minimized. Specifically indicated are (a) the kinds of knowledge concerning a proposed engine that are obtainable at various levels of R&D activity, (b) the rational starting point of a project in scope of effort and scale of equipment, (c) the decision points and considerations for enlarging the project scope and scale or for terminating the project, and (d)

the parallel effort advisable for improving the probability of success. The novel engine types are emphasized, although the conventional engine types are also discussed.

The case history of the ANP system development is reviewed to highlight some of the development concepts discussed.

II. THE STRUCTURING OF AN ENGINE PROGRAM

An item is of interest for an R&D program when it concerns (a) new and unexplored sources of energy or novel engines based either on new or conventional energy sources that promise a new flight capability or a substantial advantage over existing systems, and (b) the advancement of established systems toward high performance. A new capability may come from step improvements in one or more of the following system characteristics: specific weight (lb of weight/lb of thrust), efficiency, endurance, and ability to operate in a new environment. The step improvement in one characteristic may in some cases be accompanied by a degradation in another and may still provide the new capability.

PROPULSION-SYSTEM PARAMETERS

The growth in flight capability as related to advancement in propulsion-system parameters is illustrated by the following examples.

The turbojet engine, with a specific weight at cruise of about 1/5 that of the reciprocating engine, permitted advancement of flight speeds into the high subsonic and supersonic range up to about Mach 3, a capability unattainable by the reciprocating engine. The turbojet engine loses efficiency above Mach 3, and further increase in flight speed was made with the ramjet both because of its high efficiency beyond Mach 3 and low specific weight. At about Mach 6 to 8 the ramjet with subsonic combustion begins to lose efficiency, and studies are currently being made of the supersonic-combustion ramjet for flight in the atmosphere above speeds of Mach 8, with the hope of ultimately being able to fly into orbit at speeds of Mach 26.

The nuclear air-breathing engines, because of the extremely high energy content of nuclear fuel, promise a step advance in aircraft endurance and flight range, but no increase in flight speed or altitude over their chemical counterparts.

The chemical rocket further reduces engine specific weight to about 1/5 that of the turbojet engine, which makes possible the propulsion of vehicles to speeds of above 10,000 mph with a single-stage engine, and many multiples of this speed by firing successive stages.

Furthermore, because a rocket carries its oxidant, it can function outside the sensible atmosphere. Thus the rocket provides two new capabilities: a step advance in flight speed and propulsion in space.*

As space missions become more difficult in terms of payload, spacecraft velocity increments, and distance of destination, the efficient use of the rocket propellant takes on increasing importance in reducing mission cost and increasing mission feasibility. The solid-core nuclear rocket with a specific impulse of about twice that of the best chemical rocket (i.e., about 800 to 1000 sec versus 500 sec) promises a large reduction in mission cost for such advanced missions as large lunar logistics operations or manned Mars expeditions. Larger specific-impulse values are being projected for a number of futuristic systems: e.g., for the liquid-core nuclear rocket, as high as 1400 sec; for the gas-core nuclear rocket, up to about 3000 sec; for the impulsive nuclear system (ORION), up to about 5000 sec; and for the electrical propulsion systems, 10,000 sec and higher. The electrical propulsion systems provide very low thrust, having a very high specific weight (about 5000 lb of weight/lb of thrust), and are limited to long-duration space missions; they are, however, much farther along in development than the other futuristic space engines cited. Although little is known about the potential of the nuclear-fusion engines, there is much hope that these will also provide extremely high specific impulse.

For novel engines, the possibility of a new flight capability provokes interest irrespective of whether or not an important mission requirement can immediately be proven. For conventional engines, however, like air-breathing engines and chemical rockets, developmental proposals are usually more closely related to anticipated mission needs. For example, the belief that a need exists for a VTOL airplane currently stimulates proposals for the development of turbine engines of very low specific weight. Current interest in a bomber capable of very high speed and long range at both high altitude and sea level,

*For supersonic- and hypersonic-speed flight within the atmosphere, the ramjet is still interesting because its more efficient use of onboard propellant promises lighter vehicles and more economical operation for a given payload and range than the rocket.

and in the supersonic transport (Mach 3), has generated consideration of the associated engines. The Apollo mission generated the requirement for the Saturn 5 rocket engines. The nuclear-rocket development anticipates more difficult space missions, like the manned expedition to Mars. Interest in very-high-thrust chemical rockets, like the Nova and Post-Nova concepts, stems from the prospect of large space missions requiring placement of vehicles weighing millions of pounds into initial earth orbit. The portent of large logistics operations between earth and orbital and lunar stations has led to the study of the Aerospaceplane and other recoverable-booster concepts in search of a cheap mode of placing payload into orbit.

It has been the intention here to illustrate the factors that generate interest in proposed engines rather than to attempt to itemize all engines of interest in a complete engine program.

ENGINE DEVELOPMENT CATEGORIES

Engine development categories defined according to their technological basis may be listed as follows:

Conventional*--Scale Increase: A conventional engine for which increased scale to provide more power or thrust is the primary requirement. While improvements in major performance parameters (e.g., efficiency and specific weight) are often desired in each new development, they are not sufficient to require an advance in technology in this category.

Conventional--Technology Advance: A conventional engine with a substantial improvement specified in one or more of the major performance parameters relative to current practice. The attainment of this improvement requires an advance in technology (e.g., additional information pertinent to design improvement or better materials).

Novel--Engineering Known: A novel configuration comprising conventional components for which the component engineering data are available.

*In the above characterization a conventional engine is one of a type on which there is prior successful development experience, whereas a novel engine is the first of its kind.

Novel--Engineering Deficient: While the basic theory is understood, essential component data are lacking.

Novel--Science Deficient: The basic scientific background is lacking and, obviously, so are the engineering data on critical components.

Conventional engines requiring mainly an increase in scale over existing engines have the strongest technological background in support of their development, and this category is listed first.* Lower on the list are conventional engines that require an increase in technology to obtain advanced performance, such as a major advance in specific impulse or specific weight. Listed third but possibly on a par with the second item are the novel engines for which the technology needed for designing the components is available. The novel engines for which this technology is not available and those for which the basic phenomena are not understood are listed in fourth and fifth place, respectively, for obvious reasons.

A propulsion system employing controlled nuclear fusion, a process still beset by major basic phenomenological difficulties, is obviously in the Novel--Science Deficient category. The electrical propulsion systems, the pulsed nuclear engine, and the air-collect engines are examples of the Novel--Engineering Deficient category. The turborocket engines, which have had some exploratory development effort but no complete development, are composed of components on which there is much data and would fall in the Novel--Engineering Known class. The supersonic-combustion ramjet, the lightweight turbine engines for V/STOL aircraft, the engines with very high turbine-inlet temperatures, and the rocket engines using very high combustion-chamber pressure or unconventional fuels are a few examples in the Conventional--Technology Advance class. The F-1 rocket engine, which for the most part represents a scale increase in the engine series that includes the E-1 and H-1, exemplifies the Conventional--Scale Increase class.

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*There is the reservation that scale increase sometimes brings on new problems that require an advancement in technology.

FIVE LEVELS OF R&D

The several strata in R&D discussed in this Memorandum are defined as follows:

Scientific Research: Basic phenomena are studied in theory and in experiment.

Engineering Research: This effort is aimed primarily at procuring basic design and performance information. Test specimens rather than system components are utilized when pertinent to save time and cost; however, complete components and assemblies of components are investigated when required to evoke the phenomena under investigation. The components are usually models rather than prototypes.

Problematical Component Development:* Primary problematical components of the system are investigated. A primary problematical component is defined as one crucial to the system but with very uncertain capability of providing the desired performance. Two levels of development in this category will be discussed: (a) exploratory development, where the emphasis is on acquisition of technology; and (b) full development of a component for a specific approved engine development.

Functional System Development: Development focuses on the "stripped functional system," which is defined as an assembly of only those components essential to the study and evaluation of the performance characteristics of the total engine system.

Operational System Development: All components required for a flight-operational system are developed.

These are similar in some respects to the Department of Defense Package VI categories.[9] The major difference is the finer breakdown on the lines of test-hardware sophistication, going progressively from test specimens and models in "engineering research" to problem-

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*Although the border line between engineering research and the early phases of problematical component development is not sharp, in the latter case the components employed are approaching the prototypes for the engine.

atical components, stripped functional systems, and finally full operational engines. The objective of this breakdown is to facilitate delineation of where, in the R&D process, effort on various types of engines should start and stop to minimize investment risk. (The DOD categories tend to emphasize the classification of various proposals.)

Two research and three development strata are listed; however, exploratory problematical component development has a strong research aspect and fits into both categories.

The R&D stratum at which one starts a proposed development depends, of course, on the amount of prior information that exists on the components of the system. All engine developments naturally have their basis in technology derived from scientific and engineering research and are usually supported by a continuing program in these strata. The effort on Novel--Science Deficient engines obviously starts at the Scientific Research level and remains there until the basic phenomenological problems are solved. The Novel--Engineering Deficient engines start in the Engineering Research level and advance through the strata only when performance objectives based on the engine's needs are being obtained. Depending on the amount of background available and the pressure of time, the Conventional--Advanced Technology engine should start either in the Engineering Research or Problematical Component Development strata. The Novel--Engineering Known engine should likewise start at the Problematical Component Development level or even possibly at the Functional System Development level. For the Conventional--Scale Increase engines, a functional system is set up for test very early in the program, after initial tests of the components indicate that they are probably suitable. The development tests of the components and of the engine then proceed concurrently, the latter contributing insight into interactions between components that may require modification.

R&D PRACTICES

In order to manage R&D funds carefully, it is important to understand clearly what is acquired by the expenditures in the several R&D strata listed previously. In these R&D strata four basic types of operation are engaged in:

1. Analysis
2. Information and data acquisition through experimentation
3. Tailoring and fixing to obtain required performance and endurance
4. Performance demonstration

Analysis

In the Scientific Research stratum the frontiers of science are investigated in order that new phenomena may be revealed and understood and their application to propulsion may be explored. In engineering research the analysis is of a more applied character, attempting to indicate the fundamental relation between the physical parameters of a structure and its performance as measured by criteria like the efficiency, thrust, and endurance. Analyses are used in the development strata for designing equipment and systems and for evaluating their performance.

Information and Data Acquisition Through Experimentation

Information and data acquisition may range over such diverse items as investigation of (a) new scientific phenomena or processes relevant to propulsion; (b) the relation between the configuration of components and the associated fluid-flow, heat-transfer, thermodynamic, nuclear, or electrical processes; (c) the alloy composition and heat treatments to obtain desired materials characteristics; (d) methods of fabricating, shaping, and joining the desired materials; (e) strength and endurance characteristics of components in the desired environment; and (f) friction and wear phenomena in moving parts.

For maximum application utility these data should be documented in a manner that permits their accurate application.* Thus all physical and operational parameters that influence the situation must be reported along with the results. This requires usually a substantial amount of instrumentation for complete coverage of the pertinent measurements, and when applicable, a systematic variation of operating conditions during the test program. The utility of the data is further

*References 10 and 11 exemplify this process.

enhanced by correlation with the fundamental theory to permit a wider and more realistic application of the theory.

Data procurement proceeds most effectively in the research phases, where complete instrumentation and systematic programming are of the essence. It also occurs in early developmental phases, when adequate instrumentation is employed and problematical components are singled out for special attention. It occurs to a much lesser extent in the final full-engine development phases (the fourth and fifth R&D strata), where the contractor's overriding objective is to obtain an engine of prescribed performance within given funding and time limits.

In the full-engine-development phases the contractor believes he knows how to design the engine, and to save time and money he installs just sufficient instrumentation to indicate the engine's major performance parameters and to reveal deficiences in suspect areas. When a problem is revealed, all expedient corrective changes are applied simultaneously, and the contractor forgoes the luxury of learning which change was effective. In addition, the test runs are in the main limited to critical performance points, and again the luxury of a systematic investigation of operational parameters for the purpose of providing design information is usually not available under the time and cost constraints. In these later developmental phases much of the learning is in the nature of the art of engine development and resides in the minds of the personnel. And there are instances where this art is not applied even in the same company on a later engine development if a new developmental team is employed. A large part of whatever information is documented during the later developmental phases is often locked in the contractor's proprietary files.

What is meant by engineering information procurement will be illustrated by several examples relating to the turbojet engine. Going back 30 years, let us assume that the turbojet is in the category of a novel engine. Analysis very quickly determines that the primary critical considerations pertain to the efficiencies of the compressor, turbine, and combustor, and to the high-temperature strength of materials for

the turbine wheel and blades.* Typical research installations will be described to emphasize the extensive instrumentation, the systematic variation of design parameters and test conditions, and the limitation of the test items to just that quantity necessary to explore the phenomenon of interest.

The influence of the shape of axial-flow compressor blades on the efficiency of compression may be determined by setting up a cascade of blades in a small airflow duct. Pressure tubes are located within the blades for sensing the pressures over the blade surfaces. These tubes, connected to manometers, indicate positions of flow breakdown. Rakes of pressure tubes before and after the cascade of blades measure the airflow parameters from which blade efficiency can be determined. In these investigations, the air velocity through the cascade and the angle of attack of the airflow are systematically varied. The tests are repeated with a systematic variation in blade profile, spacing, thickness, length, breadth, tip clearance relative to one wall, etc. An attempt is made to correlate these data into generalized design curves and to relate the results to fluid-flow theory. Similar studies are made with turbine blades. In addition, a turbine or compressor stage consisting of a wheel with a complete set of blades is tested to study the effects of rotation on the flow and stress phenomena. Elaborate instrumentation may be provided to obtain, from the moving blades and wheels, surface-pressure distributions if flow phenomena are being investigated, or surface-temperature and strain distributions if strength is being investigated. In addition, extensive instruments in the gas stream before and after the test article permit detailed analysis of flow efficiency. Again the emphasis is on systematic exploration of the basic variables, covering a wide variation of the interesting parameters.

In the case of the combustion chambers, in addition to instrumentation for measuring air and fuel flow, a very large number of

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*Although the efficiencies of the inlet diffuser and discharge nozzle are important and their study should be included in a research program, enough was known about these components to indicate that they did not constitute primary problematical elements.

thermocouples are provided to measure (a) temperature distribution over the combustor wall for indications of overheating, (b) temperature distribution in the gas discharged from the chamber to indicate excessive local gas temperatures that might harm turbine blades, and (c) the temperature rise of the gas to indicate combustion efficiency. These tests might be made on segments of combustors and on complete combustors. In a typical program the distribution of the airflow into the combustor is systematically varied to indicate the effects of the location of air inlets on combustion efficiency, blow-out limits, and temperature distribution.

The critical components from a strength standpoint are the turbine blades and wheels. A study of the strength, creep, and corrosion-resistance properties of materials suitable for blades and wheels is made on test specimens over a range of temperature and stress conditions of interest and with a systematic variation in alloy composition, heat treatment, and fabrication variables. These are supplemented by the stress tests on complete turbines previously described.

The objective of the engineering research effort, as exemplified by the discussion of the turbojet engine, is to provide (a) design and performance information, (b) insight into the performance promise of the engine, and (c) some feel for the difficulty of development to achieve useful performance.

It was not the intention in this discussion to delineate a complete program for the turbojet engine but merely to attempt to provide an insight into the data-acquisition process as a basis for comparison with the tailoring and fixing process that will be discussed next. In general, research of this character is much less costly than the development of an operational engine.

Tailoring and Fixing to Obtain Required Performance and Endurance

By tailoring and fixing is meant the operation of altering the configuration to obtain the required performance and of fixing hardware of inadequate strength.

Tailoring. This requires determining the performance of components and the complete engine at specified operating points and altering the components if the desired performance is not attained. (Returning to the example of the turbojet-engine development, tailoring would be required to assure that the compressor and turbine both attain their required maximum efficiencies at the desired engine-cruise airflow rate, that compressor stall is avoided during engine start-up and acceleration, that overheating is avoided during start-up, that excessive vibration of compressor and turbine blades is avoided at important operating speeds, and that combustor flameout is avoided at desired flight-altitude conditions or during engine deceleration.) Usually the larger the number of flight operational conditions specified for a given engine development, the larger the number of test conditions that must be investigated.

Fixing. Most flight propulsion systems are pressed to the limits of the strength of crucial components in order to meet specified engine-weight limits. Failures in these crucial components may result from vibrational and thermal stresses that cannot be accurately anticipated during design and also from deficiencies in design, materials, and fabrication techniques. In addition, a flight propulsion system contains a myriad of minor parts which do not pose any serious design challenge, but some may fail because in so large a statistical assembly some defects are bound to occur. A large part of the development testing is involved in searching for and correcting defects in both the major and minor parts; a surprisingly large proportion of the failures are in the minor parts, and these are usually readily fixed.

Performance Demonstration

At several points in the developmental process when the engine appears to be attaining required objectives, demonstrations of performance and endurance are made. These include qualification of the engine for flight-testing, qualification of the engine for flight operations, and flight performance demonstration.

Performance demonstration and the tailoring and fixing process are so closely related that they will be discussed in relation to test

practices. The growth of emphasis on tailoring and fixing as one progresses further along the R&D path will be indicated.

Development Test Practices

In the research phases some tailoring and fixing takes place to obtain components in the performance range of interest for the research studies. But by and large an attempt is made to avoid much of this activity by providing ample strength in the test specimens and associated equipment except for special strength tests, in which case only the elements pertinent to the objectives of the investigation may need to be in the range of strength uncertainty.

The tailoring and fixing activities increase as effort proceeds into the developmental phases. In the early phases where phenomena are being studied, these activities are kept in hand by employing so-called boiler-plate components and by first limiting the flight-weight configurations to those elements being investigated for their strength characteristics. For example, the study of the performance of an actual set of flight-weight turbine blades need not be jeopardized by a possible underdesign of housing, shaft, wheel, or bearing also aimed at flight weight. These early developmental tests, as previously pointed out, are often characterized, as they should be, by extensive instrumentation and a systematic program to provide design information. Thus in the exploratory-engineering phase on the problematical components, both the data-acquisition and the tailoring and fixing operations are extensively involved.

When components are obtained having nearly the desired characteristics, then the functional engine system is set up, and its tests are carried on in parallel with the individual component tests. The instrumentation on any one component in an engine test installation is sparser than on the component test rig, and the emphasis shifts more strongly toward tailoring and fixing. Because interaction effects between components revealed in the system tests might require modifications of the components, an attempt is made to start functional engine tests as soon as feasible. Interaction effects between components are usually correctable with the existing state of the art.

When the contractor enters full engine development, his primary objective is to obtain and demonstrate the required performance and endurance within the allotted time and funding, letting the acquisition of design data become secondary. The contractor believes he knows for the most part how to design the engine and stakes his chances on an engine with flight-weight components, hoping that only a few deficiencies in performance and strength will be found in test. The operation is then largely in the tailoring and fixing category. As previously pointed out, because of the austere instrumentation and the ad hoc nature of test and modifications, few systematic design data come from this operation.

Flight-testing is introduced into the program when the engine has passed a preliminary flight-rating test which indicates that it is sufficiently free of defects to warrant risk of the large investment. The flight tests then proceed in parallel with the continuing ground-test program. The purpose of the flight test is to determine if difficulties will result from conditions experienced in flight, such as acceleration and vibration, from atmospheric or space environmental conditions, or from interactions with other vehicle systems. For the most part these problems once revealed can be eliminated by design changes within the existing state of the art. In a few cases where the condition cannot be adequately simulated on the ground, research in flight may be required to obtain basic engineering or scientific data. A case in point is the investigation of the effect of weightlessness experienced in space on two-phase fluid flow and heat-transfer processes. Except for these special cases of research in flight, the flight-test program is instrumented in even less detail than the ground development test and produces even fewer systematic design data.

An unsuspected disruptive phenomenon is sometimes revealed during development. Usually special research projects are then initiated to study the phenomenon in detail for the purpose of indicating methods of avoiding the associated problems, while the development effort continues with the introduction and evaluation of the fixes.

Each new engine development, even in a series of engines of a common genre, has its special component performance and fixing problems and consumes development time and funds as readily as its

predecessors. Contractor progress reports and logs give a substantially different history of failures and problems for each new development. Tables 5, 6, and 7 in Appendix A, taken from the development programs of two rocket engines (designated A and B) and showing a substantial number of tests, albeit a small part of the total program, illustrate the preoccupation with the fixing of defects that are narrowly related to the engine under development. Engine A is for a conventional liquid-propellant rocket, and the contractor has had much developmental experience on similar engines. This engine, however, is requiring as much time to develop as its predecessors and is costing more money. In a rocket, the fundamental requirements that may strain the state of the art are the specific impulse, involving the efficiency of the combustor and discharge nozzle, the combustion stability, and the cooling of the combustor and nozzle, yet the bulk of the difficulties revealed during the full engine developmental testing do not relate to these fundamental problems.

In a typical monthly development progress report, which listed a sequence of 77 rocket-engine tests and 180 tests on assorted components, the tests represented checkout and fix operations like those in Tables 6 and 7 of Appendix A.

Each new development is an adventure in new arrangements, new materials, new fabrication practices, and often new people. In the very large assembly of components, operations, and people in a flight-engine project, a myriad of defects are bound to occur. Usually these are readily fixed, but the finding, fixing, and checking represent the major part of the developmental effort. This developmental phase is more than an order of magnitude more costly than the data-acquisition activity because of the higher cost of the full-scale test items, the full-scale test rigs, and the large amount of fixing and performance tailoring that is involved in obtaining contracted performance and endurance.

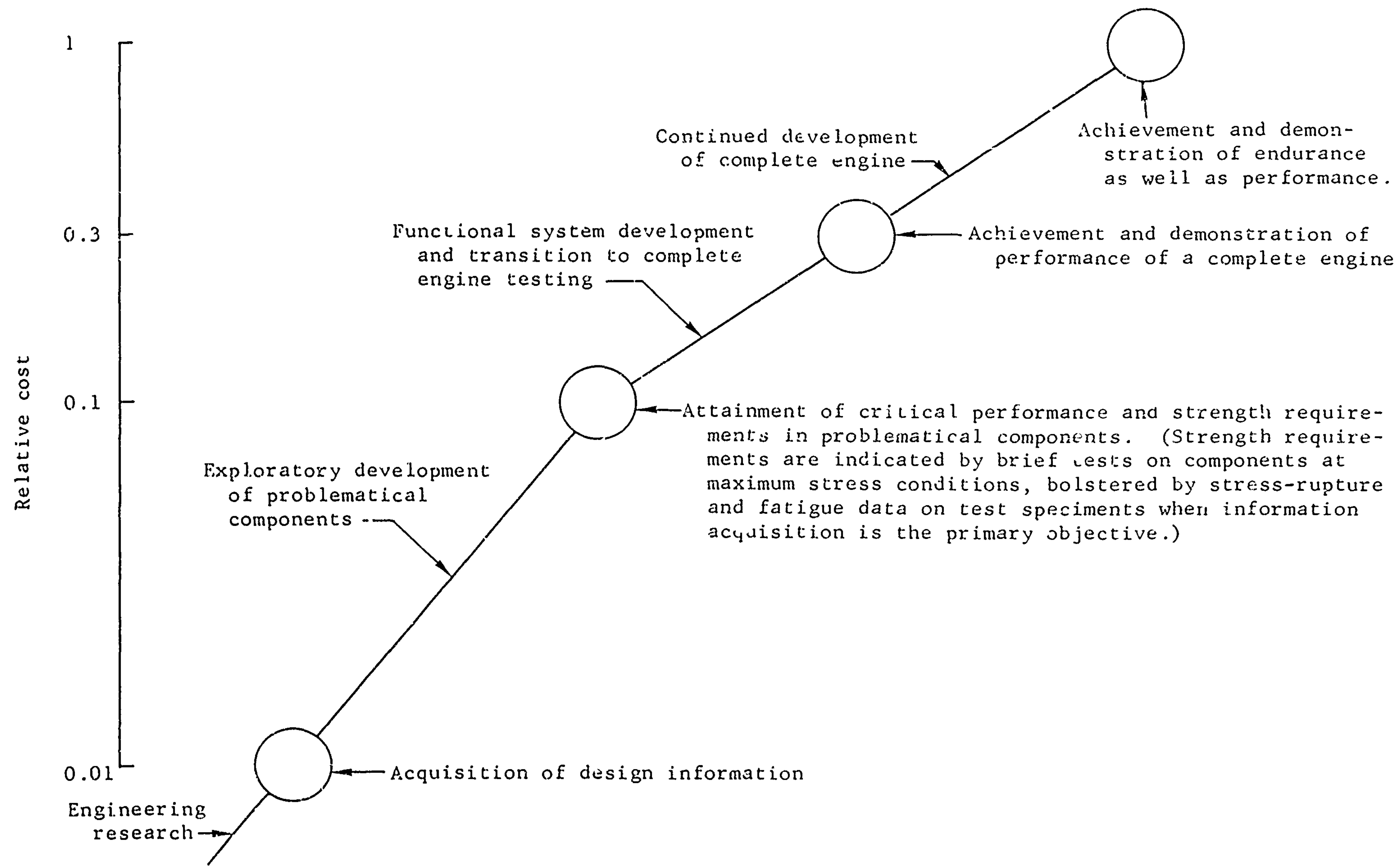

Fig. 1—Illustrative costs of several strata of R & D relative to full engine development

III. COSTS AND INVESTMENT RISKS IN ENGINE R&D

This section will attempt to place the costs of various levels of R&D activity in perspective and to indicate how engine specifications affect the depth of investment required to attain insight into whether or not investment in full development is advisable.

The engine characteristics of concern in a new development are (1) the performance (e.g., thrust and efficiency), (2) the specific weight, and (3) the endurance. While all of these characteristics are important, the distance one must go down the development path before one can attain a good insight into the probability of success depends on which of these characteristics, singly or in combination, must meet exacting specifications in order for the system to provide an advantage over its competition. This is a measure of the investment gamble.

LANDMARK COSTS

Figure 1 shows on an order-of-magnitude (i.e., logarithmic) scale the relative cost of attaining several important landmarks on the R&D path. Unity in Fig. 1 is taken as the complete development cost of an engine.* While the relative costs of achieving the several R&D landmarks shown for any given engine differ from the heuristic illustration in Fig. 1, these differences are not sufficient to alter the broad implications to be drawn from the illustration.

Lowest on the scale, roughly two orders of magnitude below full development, is the cost of obtaining design data, a process that was described in detail in Section II. These data would permit design of components for initiating a development program and would provide a good basis for estimating performance characteristics, such as thrust, power efficiency, specific fuel or propellant consumption, and cooling requirements. It would also supply information for a preliminary en-

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*This includes only ground-testing; flight tests add about 30 percent to this cost. The absolute cost values for both the ground and flight programs depend on the required engine reliability.

**The values in Fig. 1 come from a judgment rather than a detailed statistical analysis of cost histories. Statistical analyses would likewise contain a strong element of judgment in identifying expenditures with achievement landmarks. At best, estimates like Fig. 1 are very rough.

gine design from which engine weight (usually optimistic) can be estimated. Where the merits of the engine relative to its competition in important applications are not seriously altered by a degradation in weight and performance over the range of uncertainty, then these design data provide a basis for judging developmental feasibility. After an adequate study of this kind revealed that the engine phenomena can be implemented with the desired efficiency and that materials can withstand desired stress, temperature, and other environmental conditions, one would not expect any further serious technological barriers, and one would be willing to gamble that additional difficulties could be handled in a normal developmental operation.

For the initial turbojet engine, for example, the information obtained in an engineering research program would have been sufficient to indicate that it had a major advantage over the reciprocating engine for very high subsonic speed, even with a substantial allowance for uncertainty in performance, specific weight, and endurance.

Progress down the R&D path to the next landmark, i.e., "problematic component development," is indicated when at least one of the following applies:

1. A decision to develop the engine has been made. (This subject will be discussed in Section V.)
2. More accurate information for estimating performance and specific weight is needed for a decision on escalation to full development.

This additional information prior to an investment gamble on full development is needed when the merits of the system critically hinge on the attainment of performance and specific weight well beyond the current state of the art. The worth of a VTOL engine, for example, hinges on the feasibility of attaining challenging thrust-to-weight ratios. Figure 1 indicates that investments of the order of one-tenth of the expected full development cost are required to obtain this insight.

As pointed out in Section II, if information is the primary consideration, one should minimize cost in this phase by attempting (a) to focus on the problem areas of concern, emphasizing performance and strength verification, and (b) to avoid involvement in extensive fixing of incidental items. The program should be planned also to avoid

when possible the extensive alteration of components which individually show good performance to achieve matching at specified operation conditions; for example, the matching of a compressor and turbine in a proposed turbine engine.

These additional developmental activities, like performance matching, endurance testing, and defect elimination in nonproblematic items, are undertaken when a decision to develop an engine has been made and firm performance specifications have been established.

In the development of the complete engine (the next step in Fig. 1), about 30 percent of the cost is consumed in achieving performance requirements,* and the remaining 70 percent in obtaining and demonstrating endurance. This operation, as pointed out in Section II, involves a great deal of tailoring and fixing narrowly associated with the design under development. It is undertaken, obviously, when a decision has been made to develop the engine.

When the utility of a proposed engine depends on the attainment of exacting specifications for all three of the major parameters--performance, specific weight, and endurance--then it is necessary to proceed down a portion of the endurance-testing path with a substantial segment of the complete engine to obtain the insight required for a judgment on developmental feasibility. The electrical propulsion system based on the Rankine cycle is a case in point. In order for this system to show an advantage over the nuclear rocket in advanced space missions it must not only press the state of the art for performance and specific weight but must also achieve an endurance of about 10,000 hr in a given mission.(12) If this system fails to achieve this endurance within the current large uncertainty, that is, if it achieves only half or a quarter of this endurance (with specific weights above 30 lb/kw of jet power being estimated for this engine),** then the

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*The preliminary flight-test rating, which indicate that the engine is sufficiently reliable to warrant investment in a flight-test program, is obtained with a small amount of endurance-testing beyond this point.

**Based on current nuclear-turboalternator power-source technology.

electrical propulsion system will not be favored over the nuclear rocket for the advanced space missions.

The distinction between endurance required to complete a mission and endurance related to time-between-overhauls should be noted. For example, the endurance specifications on many turbojet engines relate to time-between-overhaul, which is generally large compared with the endurance required to complete one mission. Considerable latitude in time-between-overhauls has been accepted on early turbojet engines for military applications. It becomes more important when the cost of maintenance is crucial to the feasibility of the system. This may become a major consideration for some commercial ventures, like the supersonic transport, where profit is of the essence.

In summary, the inputs for a decision on whether or not to approve the development of a proposed engine should include (a) the cost of acquiring sufficient information to provide some confidence that attainment of specified characteristics is feasible, and (b) the estimated cost of development. The ratio of these two costs, (a)/(b), will be defined as the ante-factor. The more the competitive position of a proposed engine hinges on inclusion of challenging specifications on thrust, efficiency, specific weight, and endurance, the higher would be the ante-factor for the engine. For example,

Critical Challenging Specification Element	Approximate Ante-factor
Thrust and efficiency	0.01
Thrust,* efficiency, and specific weight	0.1
Thrust, efficiency, specific weight, and endurance	0.3

If directed along the lines discussed, such R&D efforts conducted prior to a decision to proceed with full development would satisfy the technological prerequisites given in DOD directive 3200.9, "Initiation of Engineering and Operational Systems Development" (see Appendix B).

*Or power.

No attempt will be made here to suggest a method of determining the absolute cost of a development program. Instead the discussion will be limited to one of the major uncertainties, namely, the number of tests required to achieve specified performance goals. One can, of course, estimate a "base number of tests" by listing all of the test conditions at which the performance should be checked. For example, in the program on a complete engine one might include tests at the conditions of thrust, flight speed, and altitude over which operation of the engine is planned (i.e., maximum speed, cruise, climb, loiter), as well as the start-up, acceleration, and shutdown sequences and endurance runs. The larger the number of important conditions set by the specification, the larger the number of tests and the cost.

However, this "base number of tests" must be multiplied by a "base amplification factor" to allow for reruns necessitated by equipment malfunction and component deficiencies and failures. This amplification factor, usually a very large unknown number, is the major reason for uncertainty and for escalation of the development time and cost. One is in a better position to estimate the amplification factor for follow-on developments on a given engine type than for a novel engine on which there is no prior development experience. Because of the natural and usually sincere optimism of contractors with regard to the base amplification factor, it is not surprising that most programs require extensions in time and cost.

FOCUS AND SCALE

It is with novel engines that a breakthrough may occur, leading to a new and important flight capability. But because a point of breakthrough cannot be forecast, a broad program for exploring many promising possibilities for implementing attractive concepts is advisable. While in general one must subscribe to the merits of a broad research program, some focusing of effort should be considered; namely, as long as major uncertainty exists regarding crucial elements of a novel engine, an extensive effort on other portions of the system is not warranted unless they are of interest per se.

A case in point is a propulsion system utilizing controlled nuclear fusion. The phenomenon of fusion has been amply demonstrated; in fact, the H-bomb employs the fusion process, and some trace amounts of fusion have been reported in controlled-fusion experiments. However, it is still not certain that a technique can be devised to retain the fusile material at the millions of degrees of temperature and at the proper density for a sufficient time to achieve a significant amount of controlled fusion Until this central uncertainty is resolved, it should receive the emphasis in any funding for this propulsion system, and little should be spent on ancillary portions of the system.*

Somewhat closer to practicality are those novel systems where both the central phenomena and physical processes for controlling the phenomena are understood, but where there is major uncertainty as to whether or not (a) the phenomena can be controlled with the required efficiency, and (b) materials can withstand the operating conditions. The effort at this point should be aimed at the primary problematical components.

In the development of a novel engine, investment risk and R&D costs can be reduced if the initial engine scale and its growth during the developmental process are carefully planned. To facilitate the discussion two hardware scales will be defined:

1. Minimum representative scale--This is an engine or component size chosen to economize on hardware, facility, and test costs, but still capable of providing useful design information and a realistic insight into the performance capability of a useful engine.

2. Mission scale--This is an engine or component size chosen for a mission application.

If there is a very large increase in cost for developing the mission-scale system, then time and money would often be saved by choosing the minimum-representative-scale system for the initial R&D on a novel engine. The use of a modular-design concept may be feasible in some cases to transform a minimum-representative-scale system into a mission-scale system, and this should enter into the planning

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*The importance of focusing the current effort on the plasma-stability problem, which appears to be the key to obtaining the required plasma confinement, is discussed in Ref. 13.

considerations. This is particularly desirable if there is some uncertainty regarding the initial mission requirements. Even if a firm set of requirements were specified, and the pressure of meeting the established operational date required that the initial engine developed be of mission scale, then at least in the research and early exploratory phase minimum-representative-scale components should be used. Such components would facilitate learning and would minimize the investment loss if tests revealed basic reasons for reorienting the design or terminating the project. The effort in these phases should be sharply focused on the problematical components. When tests indicate the feasibility of obtaining desired performance objectives, the scope and scale can be increased.

Likewise, when major advancements in technology are required with conventional engines, the use of minimum-representative-scale components in the early R&D phases often reduces learning cost. Once an understanding of the technology is achieved, the development can proceed immediately to the mission-scale engine.

IV. ENGINE R&D WITH AND WITHOUT ESTABLISHED MISSION REQUIREMENTS

JUSTIFICATIONS FOR R&D

The extent of the R&D activity depends on the strength of the justification for the system; we may choose to buy only the engineering data needed to design an improved system in anticipation of a future requirement, or an improved component ready for application, or a complete engine.

There are several levels of justification for R&D effort:

Established mission requirements: The strongest support for the development of an engine is provided by the existence of either a military mission requirement or, in the case of developments for NASA, an approved space mission which needs the proposed system.

Broad utility: Although a requirement as defined above may not exist, an engine or a component may promise such broad utility that its development may be justified on that score.

State-of-the-art advancement: Engineering research, or possibly the development of a component or an engine, may be justified if it provides a new or improved performance capability, even if at the moment an application requirement does not exist. Each case, of course, must be judged on its own merit, with careful consideration of development cost and possible utility. The existence of a continuing important application justifies product improvement.

Economic advantage in established missions: Convincing indication of a significant economic advantage over present engines in an established operation can justify development of a proposed engine.

Full development of an operational engine is expensive; it raises the question, Does the nation need its application? System cost-effectiveness and mission analyses are generally utilized to determine whether the proposed engine will perform an important mission at a lower cost than competing systems or that other engines cannot. These studies can become very complex, involving consideration, in some of the military cases, of the complete force structure, the enemy's anticipated force structure, and a postulated scenario of conflict.

Thus, for example, a new bomber finds itself competing with ballistic missiles for strategic applications. In spite of the reservations that usually enshroud the results of mission-justification studies, it is essential that these be done for major development proposals. For example, after the investment of $200 million in Project PLUTO toward demonstrating that development of the nuclear ramjet was technically feasible, the project was canceled because of lack of proof of a military need adequate to buttress a projected multibillion-dollar development. If it had been appreciated that Project PLUTO was fated to become a contribution to advanced reactor technology for some possible future application, then the program could have been planned to obtain this information at a lower cost. A timely evaluation of the military worth of the system would help to establish the scale and extent of the R&D effort.

It is even more difficult to provide a cogent rationale for space missions; they are usually established by decree based on such tenuous considerations as national prestige, the advancement of our space technology with the prospect of discovering a military application, and the acquisition of scientific knowledge per se. For example, Apollo is an established mission, and the associated system developments receive full support. While there is much interest in the space station, the lunar base, and manned exploration of the near planets, Mars and Venus, these do not have the status of established programs. A great deal of associated developmental activity has nevertheless been generated on the faith that the continued exploration of space is in the national interest.

Without a decision to proceed with the development of a proposed system, the procurement of design information may still be desirable as a hedge against the possibility of a sudden and urgent mission need, provided that one of the above justifications for the engine can be shown. Because of the low cost of engineering research relative to that of development, a broad program at this level has been advocated. How far one goes beyond research in the absence of a requirement for the engine, currently one of the critical issues, will be discussed in detail later, and the associated role of judgment will be explored.

The prospect of a substantial net economic gain from a large, well-established, and continuing flight operation can be a strong justification for development of a new engine. On the other hand, if the extensive flight operation is a projection into the uncertain future, if it is poorly defined, and if many optimistic assumptions are required to substantiate the cost advantage of the proposed engine, then the economic-gain justification is weak. Many recoverable-booster proposals, for example, have foundered for this reason. When one looks to the distant future, effort to provide a new flight capability is more justifiable than effort to reduce cost predicated on traffic-volume estimates.

THE ABSENCE OF A REQUIREMENT

Novel propulsion systems usually create a dilemma arising from the following: (a) the engine promises interesting advanced flight capabilities, (b) estimated development costs are high, (c) at present no requirement for the associated mission has been established, and (d) estimated development times are long. The anticipated long lead time generates pressure from proponents of the novel engine for immediate program initiation in order that engine development may be nearer completion should an application requirement suddenly be generated; however, in the absence of a requirement, approval for the associated large appropriations is very difficult to obtain.

There is great concern that our current insistence on a mission requirement is unduly hampering our engine-development program and that there is room for engine developments supported mainly by the intuition of wise decision-makers blessed with much foresight.

James T. Ramey reflects this viewpoint in the article, "The Requirements Merry-Go-Round: Must Need Precede Development?":(14)

> A problem, which has been with us for the past eight years, is threatening to restrain the forward movement of much of our atomic energy program. This problem arises from the practice of holding back the development of new hardware until a specific mission or requirement is formally established by the agency that would use the hardware, such as the Defense

> Department or NASA. This practice has prevailed despite the fact that the agency very often is not in a position to establish such a requirement unless the program is permitted to move forward to the point of demonstrating its value. On the one hand we are told we must have a formal requirement before we can proceed with the development of a device and, on the other hand, we have difficulty finding a requirement until we have demonstrated a developed device. Congressman Melvin Price (D, Illinois) calls this chicken-and-egg dilemma the 'requirements merry-go-round.'

He cites the development of the nuclear submarine as a case in point,

> Submarine nuclear propulsion plants which revolutionized underwater warfare were developed under Admiral Rickover's direction. The combination of the nuclear submarine and missiles equipped with nuclear warheads in Polaris-type submarines represent one of today's most effective weapons systems for the defense of the free world. Yet this application of reactor and warhead technology was not conceived by anyone at the time the work was carried forward in the late forties and early fifties.

When development costs are low or moderate, we are freer to gamble that a new capability may eventually find application, but when faced with developments in the billion-dollar class, much more careful planning is required. In the absence of a firm application requirement, we cannot completely ignore the novel propulsion systems which promise an advanced flight capability, while on the other hand an arbitrary policy of funding engine developments on all interesting proposals would place an unacceptable burden on the budget. Under these circumstances, the question of how far should one go down the R&D path becomes paramount.

Exploratory efforts involving the acquisition of design information are initiated on a large number of promising engines with the understanding that most of them may not prove worthy of advancement to full development. Hence a definite point of review should be programmed for each exploratory effort, allowing a reasonable time for acquiring good understanding of the technical problems and insight into the engine potential. If by that time no application requirement has evolved to warrant the expected developmental cost, then the

effort should be curtailed after the desired information has been acquired, unless there are important mitigating considerations, which will be discussed later.

When a contractor or a governmental agency is provided with a very expensive laboratory in support of a given engine program, the large uncertainty that the engine will advance to full development should be recognized. To avoid the risk of a short life, the laboratory should therefore be established as a more general advanced-technology facility with a directive to plan in the area of its application for a continuing program in advanced concepts. The closing down of a laboratory when the contract for the initial engine is terminated wastes not only funds invested in setting up the plant and its test equipment but also in assembling the staff and organizing an efficient operation (a process that requires about three years).

The next step, considerably higher in cost, would be the prototype development of the critical components. If a mission requirement does not evolve for some time after this development, then there is considerable risk that the developed component will not be used because (a) changes in application concept may impose a new set of requirements,* and (b) advancement in technology and materials in the interim may obsolete the developed component. The new components would have their special tailoring and fixing problems. When the development team dissolves, a substantial part of the development art that resides in the minds of the staff is dissipated. For these reasons the full development of an expensive novel component (or engine) without a mission requirement represents in most cases an unnecessary investment risk.** (Variations on this theme will be discussed later.)

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*For example, during the development of the ANP system, the position of the high-speed penetration bombers was undermined by the ICBM and the defense missiles, with the result that interest in the nuclear engine began to shift toward its high-endurance capability for patrol, command and control, and antisubmarine applications. This shift would change the engine specifications.

**Some overzealous protagonists of a proposed engine, by pressing prematurely for a very expensive full development without a mission requirement (which brings the whole concept to an issue at a most vulnerable time), have been rewarded by cancelation of the project.

Moderate developmental costs and flexibility of the engine concept have led to some adventuring in exploratory development without a requirement. For example, in the field of advanced turbojet engines, uncertainty with regard to a requirement for a new manned bomber, supersonic transport, or V/STOL airplane has led the engine companies to gamble on the development of a gas-generator core consisting of a compressor, combustor, and turbine which can be used as a turbojet engine and which, by adding fan, compressor, and turbine stages, can be adapted for many applications. Thus they hope, with a modest investment, to be in an advanced state of developmental readiness regardless of how the mission requirement evolves. This approach has merit where commonality in design does not introduce significant loss in performance compared with engines designed specifically for the missions.

Rocket engines, like the turbojet, have arrived at a point where continued increase in size cannot be categorically predicted. There will, of course, be a continuing upgrading of engine performance through increases in combustion-chamber pressure, advancements in propellants, stronger casing materials, and improved nozzle design--all of which are in research. However, the need for a rocket engine having an order-of-magnitude-higher thrust than the F-1 would depend on whether extensive space-transport operations will be approved to follow the Apollo mission. Since development of an engine of this size could represent a billion-dollar investment, the previous remarks concerning the level of effort justified in the absence of a requirement again apply. In this case, because the understanding of combustion instability is inadequate, the large combustor represents one of the crucial problematical components, and it should be included in a research program aimed at hedging against a possible future need for larger engines. This program should again be limited to phenomenological investigations and should not include "debugging" of a flight-weight combustion chamber in a developmental type of operation.

Although full development of a component or engine without an application requirement is in the main not advocated, the national policy, as pointed out by Ramey, cannot be so narrowly circumscribed. The R&D problem is sufficiently complex that all possible cases cannot be adequately

adjudicated by a simple set of rules, and one suspects that there is much room for judgment. The future safety and strength of the nation is worth the possible risk of funds in an occasional developmental gamble on an engine having much potential but no immediate requirement. However, the concurrence of at least the following criteria should be required for a favorable judgment:

1. Design information on the critical components is on hand, and evidence of developmental feasibility stemming from these data is clear.
2. A step advantage in future missions is indicated by analyses.
3. An attractive initial application can be described that requires engine performance within the compass of the available design information.

The lack of any one of the three criteria listed weakens the case for a proposed development. The application of these criteria limits very substantially the number of engine proposals that require a decision on full development. For example, as was previously pointed out, the promise only of a future cost advantage on the basis of very uncertain and optimistic assumptions, as for some of the expensive recoverable-booster proposals, does not provide an adequate basis for an investment gamble on full development. The supersonic combustion ramjet is a case where a clear advantage at hypersonic flight speeds within the atmosphere is indicated but where further design data and a better definition of an initial useful application to generate performance requirements are needed before a decision to proceed with an engine development comes to an issue.

The nuclear-rocket development is a case where the criteria previously mentioned were substantially met. Research information had indicated developmental feasibility,* and mission analyses had indicated

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*The primary problematical component was the nuclear reactor. The research data on the fuel elements on which feasibility was judged pertained to heat transfer, thermal conductivity, chemical and structural stability at high temperature and under radiation, thermal shock, and resistance to erosion and corrosion by the high-temperature hydrogen stream.

a clear major advantage for the nuclear rocket over chemical rockets in ambitious space missions (e.g., manned planetary expeditions or large logistic operations in support of a lunar base) and had provided some insight into the performance requirements for the initial engine. With these indications, nuclear-rocket development was initiated through an arbitrary decision, based largely on faith, that an expanding space effort is the way of the future and that these ambitious space ventures are bound to be approved.

In the final analysis, after the mission-evaluation results and the data supporting feasibility are on hand, the decision on whether or not to proceed with development of a novel engine requires much judgment, involving the weighing of intangibles like the probable civil and military worth of the proposed future missions relative to their cost in the context of the competing demands on the R&D budget. Included in an exercise of judgment is often an element of faith.

An expression of faith is largely subjective, arising out of a synthesis of the believer's experience and intellectual background, his understanding of the gains, costs, and risks, his sense of the flow of history, and a philosophical and often largely intuitive process of projecting from this base into the future. The support of the Apollo systems and of the nuclear-rocket development rests on faith in a burgeoning space age. The following are some additional examples of expressions of faith that have been advanced in support of engine developments:

- A need will always exist for better manned interceptors, bombers, and reconnaissance airplanes.
- There will eventually be a nuclear-propulsion airplane exploiting the extreme flight endurance made possible by nuclear fuel.
- There will be a continually increasing space power requirement that will absorb any level of power that can be developed.
- A need will be found for an efficient engine for atmospheric flight at hypersonic speeds.

Making such expressions of faith is a highly valued art. No attempt is made here to evaluate the above expressions. The purpose of this discussion is rather to indicate guidelines and to show that there is a logical procedure for adventuring into new propulsion areas which tends to reduce investment risk.

FULL ENGINE DEVELOPMENT

The contracting agencies for engine development are continually confronted by flight tasks of increasing difficulty which impose ever more difficult requirements on the engine. For this reason, and because of the stimulus of competition, contractors press beyond the state of the art. A great deal of judgment is required on how and where to adventure.

A development may fail to attain its objectives at the expiration of its contracted funds and time because (a) the performance goals called for unreasonable extensions of the state of the art, (b) unexpected difficulties were encountered, and (c) the contractor and his design concepts were inadequate.

It is not unusual for most developments to run into unexpected difficulties. If it appears that these problems are being overcome, that success is imminent, and that the engine is still useful, then extension in development time and funds is generally granted. However, if at the date of reckoning, the engine is still encountering much difficulty and a competitor's engine is showing more promise, or the application has disappeared, then the engine development is canceled.

Engine components are generally designed light with the hope that only a few are inadequate in strength. When these inadequate components are uncovered in the development program, they are made stronger, usually with some increase in weight, and in this way one approaches a functional lightweight engine. Thus engines usually grow heavier as the development program proceeds. If the final engine does not meet the performance goals originally specified but does perform satisfactorily at a somewhat lower level, it may still be a useful engine. An increase in specific weight or specific fuel consumption may be

compensated by a reduction in payload or range. Although the better performance would be preferred in some applications, the reduced performance may still be acceptable.

However, if the application for which the engine is intended is critically dependent on attainment of the specified performance objectives, then a substantial miss of an objective might jeopardize the entire project. The supersonic transport is a case in point. The range is set by the terrestrial geometry of the points of call, and the passenger load is set by the economics of the operation in the context of its competition. If the engines are too heavy, one could of course offload passengers. An excessive specific fuel consumption could also be compensated by offloading passengers, provided that the airplane is designed to accommodate the extra fuel. In any event, an airplane development that has no allowance or tolerance for the contingency of some degradation in attainable engine characteristics would be extremely risky.

The desirability of funding the development of critical problematical components prior to initiating full engine development when a major advance over the state of the art is being specified has been discussed. When achievement of a scheduled operational date is vital, and when other investments like airframe development hang in the balance, it may be expedient to reduce the risk in engine-contractor performance by supporting more than one design concept with competitive contractors. A choice of concept for full development following some background of problematic component development enhances the probabilty of success. When warranted by the importance of the application, and when challenging engine endurance is a crucial requirement, it may be expedient to carry more than one promising design concept into full development.

Ultimately the capability of an engine must be proven in flight test. Flight-testing is expensive because of the high cost of the flight vehicle, associated flight systems, and flight operations, and the slow test pace caused by (a) lengthy preparation of the large amount of equipment, in addition to the test engine, that must be brought to a high level of reliability and (b) delays introduced by

inclement weather. Thus the cost per test can be an order of magnitude larger in flight-testing than in ground-testing. Flight-testing should not be employed for finding and eliminating those defects which could have been found in a ground facility. Premature initiation of the flight-test program risks greatly increasing the cost of a development by inviting many flight failures.

In a number of proposals for engines where much controversy is expected, the development plans often contain a very early flight demonstration of feasibility following an austere ground-test phase. If by chance the engine succeeds in this flight test, an extensive development operation in a ground facility to achieve specified performance, endurance, and reliability goals is still required. On the other hand, failure of the flight test is not proof that the proposed engine is infeasible nor that the design is basically unsound; failure must always be expected in the flight test of an advanced system early in its development and may come from causes that can be readily corrected. Premature flight-test proposals stem not from the logic of development planning but rather from program-sales motivations.* It is an unwarranted early investment risk in a design or structure that may be inadequate.

When ground-test facilities for developing a proposed engine are not available and their construction is very expensive, the expedient of employing flight tests is often suggested. The extensive tailoring and fixing operation required to develop an advanced engine in flight is exorbitant both in cost and time. Extreme cost of a ground-test facility may be an adequate reason not to develop a proposed engine, but it is not a good reason for substituting development by flight-testing. If an adequate ground-test facility cannot be conceived, then only an engine of very exceptional potential worth would justify the very costly and lengthy development by flight-testing--a case that would be difficult to make for any of the current novel-engine proposals.

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*Consideration of the merits of early flight demonstration as a program-promotion device in the highly competitive world of R&D is beyond the scope of this Memorandum.

V. AN ILLUSTRATIVE REVIEW OF THE AIRCRAFT NUCLEAR PROPULSION SYSTEM DEVELOPMENT

The ANP project was one of the first of the novel systems having a high development cost. It was allowed to proceed to an advanced state of development, and its extensive history permits highlighting of many of the principles discussed in this Memorandum. It is not the intention of this discussion to criticize the management of ANP; the situation was new and complex and wisdom came through hindsight. It is presented here for its instructional value. An extensive review of the ANP program, from which the data in this section were taken, is given in Ref. 15.

A résumé of the historical portion of ANP pertinent to this discussion is shown in Table 1. The initial investigation of nuclear propulsion, designated the NEPA* project, largely a feasibility study, was terminated in 1951 and was replaced by the development project known as ANP, supported by Air Force and AEC contracts to General Electric and Pratt & Whitney for the propulsion system, and Convair and Lockheed for related aircraft efforts.

General Electric received the major part of the funds for development of the system, comprising a turbojet and a direct air-cooled reactor. Pratt & Whitney was supported at a lower funding rate as a backup effort on systems employing an indirectly cooled reactor. They studied a circulating-fuel reactor (CFR) in collaboration with Oak Ridge National Laboratory, a supercritical water reactor, and a lithium-cooled reactor and chose the latter for their subsequent developmental effort. General Electric initially estimated that the first power plant would be delivered to Convair in May 1956 at a program cost of about $188 million.

The direct air-cooled system was chosen for the primary effort because it was the simplest system. It comprised a turbojet engine in which the reactor replaced the combustion chamber and the air from the compressor was heated directly by passage over the reactor fuel elements. In the indirectly cooled reactor systems a liquid reactor

*Nuclear Energy for Propulsion of Aircraft.

Table 1

SOME LANDMARKS IN ANP DEVELOPMENTAL HISTORY

Date	Item
May 28, 1946	NEPA project initiated under Fairchild
April 30, 1951	NEPA terminated and responsibility transferred to GE, who established direct air-cooled cycle as their choice in fall of 1951
September 1952	P&W started study of indirect cycles including supercritical water-reactor system and ORNL started CFR investigation
December 1953	P&W abandoned supercritical water and concentrated on CFR with ORNL
February 1955	AF instituted WS 125A, and design of XMA-1 power plant was initiated shortly afterwards
January 1956	GE started testing HTRE 1 with a J-47 for air supply
Mid-1956	Construction started on a CAMAL test facility for P&W
January 1957	AF cancelled WS 125A
July 1957	Testing started on HTRE 2
August 1957	AF withdrew support from P&W[a] AEC stopped work at ORNL on CFR and shifted effort at P&W wholly toward lithium-cooled reactor system
Early 1958	First run on X-211 turbojet engine on chemical fuel[b]
Mid-1958	First test on HTRE 3, which continued to end of 1960
Early 1960	GE reoriented its effort to ceramic reactor
Early 1961	ANP canceled

[a]P&W continued development of the J-91 engine (400 lb/sec airflow) on chemical fuel until it passed the 50-hr test, at which point its development was terminated.

[b]The GE X-211 engine (400 lb/sec airflow) had accumulated 214 hr of operation of four engines employing chemical fuel by March 1959.

coolant was used which transferred its heat to the air in the turbojet engine through a heat-exchanger system consisting of a liquid-to-liquid loop and a liquid-to-air loop.

The ANP program was characterized by the many changes in direction indicated in Table 2. However, the scale of the effort and the developmental planning stemmed largely from an Air Force statement to the AEC in December 1953 that there was an urgent need for nuclear-powered aircraft, and from the establishment of Weapon System (WS) 125A in November 1954 as an official Air Force requirement. General Operational Requirement (GOR) 81, issued in March 1955 relative to WS 125A, specified a cruise speed of not less than Mach 0.9, supersonic speed in the combat zone, and a date of 1963 for availability of operational units. In June 1955, the AEC and DOD agreed to accelerate the ANP program to enable testing of a prototype propulsion system in about 1959.

Table 2

A SUMMARY OF THE MAJOR CHANGES IN PROGRAM EMPHASIS AND DIRECTION(15)

Program Emphasis	Period From	Period To	Length of Time (months)
Flight-demonstration program (X-6)	April 1952	May 1953	13
Applied R&D	May 1953	November 1954	18
WS 125A program	November 1954	December 1956	25
Experimental development program--no flight objectives	January 1957	March 1957	2
Experimental development program--flight objectives	April 1957	February 1958	10
Development program--flight objective in militarily useful aircraft	March 1958	October 1958	7
Development program for CAMAL mission	October 1958	July 1959	9
R&D program	July 1959	March 1961[a]	20

[a]ANP termination.

These requirements established the large size of the engines, the high turbine-inlet air temperature to give the high cruise speed, and concurrent development of the reactor and the other turbojet components to meet the operational date. Although WS 125A was canceled in December 1956, the engine and reactor requirements and the developmental funding pace continued until termination of ANP in 1961 on the momentum established by WS 125A (see Table 3). Following cancellation of WS 125A, the Air Force substituted as its objective for ANP a missile carrier designated CAMAL* that was capable of long-endurance patrol and could fly near sea level (i.e., 500-ft altitude) at a speed of about Mach 0.83 to 0.9 for alert missile patrol, penetration bombing, and reconnaissance. CAMAL never attained formal approval by the DOD as a weapon system. GOR 172 issued by the Air Force relative to CAMAL set target dates of 1962 for flight demonstration of an airplane propelled by a prototype nuclear engine, and 1966 for the weapon system to be operational in the Strategic Air Command. Both GOR 81 and 172 remained in force until superseded by Advanced Development Objective (ADO) 20 in November 1960, which reduced the project to an exploration of advanced reactor concepts.

For the most part, the reactor development was funded by the AEC, and the remainder of the engine by the Air Force (see Table 4). The Air Force also funded reactor-shielding studies and airframe analysis and research. The turbojet engines were to be developed on chemical fuel in parallel with the reactor. In the General Electric development the complete engine was designated the MXA-1 and was to consist of two X-211 turbojet engines connected in parallel to a common reactor.

All of the elements of the drama have now been indicated:

1. A system containing a novel <u>primary problematic component</u>, namely, a high-temperature, aircraft type of nuclear reactor on which there was no prior developmental experience
2. A difficult engine-performance objective imposed by the requirement for high flight speed

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*Continuously Airborne Missile Launcher and Low Level Weapon System.

3. A very large engine size set by a weapon-system requirement
4. An early scheduled operational date which provided the pressure for concurrent engine and reactor development
5. An early definition of the weapon-system application

All of these elements conspired to make the ANP program very expensive and a very-high-risk operation.

Table 3

ANP COSTS BY AGENCY(15)
(Millions of dollars)

Fiscal Year	Cost by Agency AEC	Cost by Agency USAF	Cost by Agency USN	Total Cost	Comments
1946-51	7.46	19.95	1.50	28.91	NEPA project
1952	11.25	7.78	0.50	19.53	Start of ANP with contracts to GE, P&W, Convair, and Lockheed
1953	20.93	22.96	0.18	44.07	AF informed AEC there is highest priority, December 1953
1954	23.93	11.94	0.10	35.97	....
1955	27.48	16.83	0.49	44.80	WS 125A established, November 1954
1956	49.41	38.06	3.72	91.19	First test of HTRE 1, November 1955
1957	79.15	99.38	1.46	179.99	WS 125A terminated, December 1956
1958	73.12	103.63	2.56	179.31	First test of HTRE 2, August 1957, and of HTRE 3, 1958
1959	76.40	79.13	1.77	157.30	CAMAL established, October 1958
1960	69.18	63.34	1.77	134.29	CAMAL terminated and project shifted to advanced reactor development, July 1959
1961	69.29	54.53	....	123.82	ANP terminated, March 1961
Total	507.60	517.53	14.05	1039.66	....

Table 4

ANP COST BY APPLICATION FROM 1946 to 1961 (15)
(Millions of dollars)

Main Area of Funding	Propulsion System: Direct Cycle[a]			Propulsion System: Indirect Cycle[b]			General			Total
	AEC	USAF	USN	AEC	USAF	USN	AEC	USAF	USN	
Reactor	299.15	......	....	130.03	.....	....	.....	......	....	479.18
Engine other than reactor	......	262.06	1.56	......	98.48	1.84	.....	......	....	363.94
Feasibility studies and general support	......	......	....	......	.....	....	28.41	40.83	4.91	74.15
Airframe, subsystem, and component design, and related shielding and radiation-effects studies	......	......	....	......	....	....	.....	117.36	5.72	123.08
Total	......	......	....	......	.....	....	.....	......	....	1040.35

[a] Utilizing direct air-cooled reactor.

[b] Utilizing indirectly cooled reactor.

Three reactor versions, HTRE 1, 2, and 3, had been built by General Electric and tested, and the X-211 turbojet engine was in an advanced stage of development, operating on chemical fuel. However, the reactor and engine had not yet been mated for test of the complete nuclear propulsive system when ANP was canceled, after an investment of $1 billion, in March 1961. A turbojet engine for the indirectly cooled reactor was likewise in its final development stage at Pratt & Whitney, although a reactor for this system had not yet been constructed.

The gamble on concurrency had not paid. The reactor was far below the promised performance. When this was apparent just before the cancellation of ANP, the Air Force ordered (through ADO 20) that effort on the metallic, direct air-cooled reactor be terminated and the funds applied toward the lithium-cooled reactor and the ceramic air-cooled reactor. Each of these reactors would require a development time of probably more than five years; thus the investment of about $500 million by the Air Force on the turbojet engines and on other nonreactor items (Table 4) proved to be premature.

Was there a reasonable basis for this gamble on concurrency? Prior to the construction of the first test reactor, research on nichrome fuel elements and other reactor components indicated that the proposed propulsion system could fly an airplane. However, the small margin between the highest allowable fuel-element hot-spot temperature and the desired average air temperature at the reactor exit, requiring very careful distribution of reactor power and airflow to provide the high performance of interest to the Air Force, identified the reactor as the <u>primary problematical component</u> and gave warning of a lengthy reactor development. (In contrast, the operational conditions for the compressor and turbine for the desired flight performance were well within the existing state of the art.) The difficulty of obtaining a high reactor-discharge-air temperature was confirmed in the subsequent tests on the three reactors, HTRE 1, 2, and 3.* Reactor tests started in November 1955 on HTRE 1 and terminated in December 1960 on HTRE 3.

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*HTRE 1 was a water-moderated reactor. HTRE 2 was also water-moderated but was provided with a test chamber for installation of experimental fuel elements. HTRE 3 contained zirconium hydride as moderator and approached a flight prototype.

The final tests on HTRE 3[16] resulted in an air temperature considerably lower than that desired for military application, and an improvement of less than 200°F over HTRE 1.

	Endurance Run (126 hr)	Elevated Performance (20.3 hr)
Mixed-core discharge-air temperature, °F	1330	1370
Maximum fuel-elements temperature, °F	1900	1986

Thus at the issuance of ADO 20 in November 1960, which arranged for the termination of effort on the metallic, air-cooled reactor, it was evident that the development of this reactor to obtain the desired air temperature would be very difficult and long. At no point in this reactor history was sufficiently interesting performance obtained to warrant extension of the program beyond a reactor development. The argument that the turbojet engines developed under ANP or some of their components might eventually find other applications, and hence might not represent a total loss, is too tenuous a justification for risking funds in their development.

The reactor deficiency was officially recognized as early as October 1956 when the Assistant Secretary of Defense, Research and Development, advised the Secretary of Defense that

> The scope of the nuclear-powered supersonic aircraft system be changed to that of a research program, oriented to realize the radical improvement necessary to make a nuclear-propelled aircraft system which was a major advance over a chemically powered aircraft system.
>
> All phases auxiliary to the demonstration of reactor feasibility be deferred, i.e., engines and unessential facilities.
>
> As the success of the above research activities warranted, system studies and engineering feasibility determinations be made to establish whether a nuclear-powered aircraft would be a major advance over a chemically powered aircraft.

> Further development of a nuclear-powered aircraft for service u e be deferred until research, component development, feasibility, and system studies all indicated concurrence that nuclear propulsion should be employed.

However, as pointed out in Ref. 12, the DOD was somewhat slow in implementing its conclusions, and both reactor and engine development continued substantially until termination of the project in 1961.

The question of policy on the development of expensive engines without an established military requirement was sharply brought to light by the ANP project, and it is still largely unresolved. The continued effort of the Air Force to tie this development to a weapon system reflected the impression that projects involving hundreds of millions of dollars would not otherwise receive DOD support. The DOD's conservative philosophy on expensive projects without a weapon-system requirement was probably also shared initially by many elements of the Air Force command; however, evidence of some liberalization of the point of view of the military occurred toward the end of the program. This was indicated by the following statement by the Deputy Secretary of Defense summarizing the guidance on ANP received from the Joint Chiefs of Staff on July 19, 1959:

> Briefly stated, the Joint Chiefs of Staff expressed their conviction that there is considerable military potential in the nuclear-powered aircraft and that early achievement of the capability for nuclear flight would be in the national interest. They stated, however, that they were unable at this time to establish a military requirement for nuclear-powered aircraft or to define the specific weapon system for which it would be used. With respect to the future course of the development program the Joint Chiefs of Staff advised that the present program should be extended to include flight test as soon as technically feasible. The test vehicle selected should be capable of testing any of the engines that may be developed and the program should enable the application of advances of reactor technology as they occur.

During the ANP project, the advent of the ICBM weakened the position of the bomber as the principal Air Force strategic weapon-delivery system. In fact, since the B-58 the Air Force has not been able to make a successful case for a new bomber, as evidenced by the resistance

of the DOD to the B-70 and other proposed advanced manned strategic aircraft. Thus, even if the direct air-cooled nuclear turbojet engine achieved the desired design performance, an operational bombing force based on this system would probably not have been approved by the DOD. Sizing of this engine to the very high thrust needed for the bomber application resulted in high cost of hardware, test facilities, and operations. In applications for ANP now being discussed emphasizing the long-endurance capability of the system (e.g., missile patrol, command and control, and antisubmarine warfare), much smaller engines and much lower turbine-inlet temperatures could be used than were required for the bombers. From this point of view the development difficulty would be eased. But these applications would probably also require completely shielded reactors, possibly calling for development of the more compact but more complex indirectly cooled reactor system. Thus the lesson that must be borne in mind in planning for any novel propulsion system which may require a very long development time is that possible changes in application concepts may drastically affect design requirements for the engine. Thus application-oriented decisions that involve costly commitment should not be made prematurely.

Apropos of this evolving uncertainty in application, the Director of Defense Research and Engineering transmitted the following comments on July 7, 1959:

> In our opinion, no possible (within reason) ANP development program can lead to an operational capability which the military could depend on for important and useful missions before approximately 1970. Since no one can foresee what the military situation will be at that time, it is not possible to describe in any detail what ANP will be used for, although a number of disparate possibilities, including CAMAL, logistics, and ASW or AEW/C surveillance, have been proposed. Similarly it is not possible to "prove" as is sometimes attempted, by means of cost effectiveness studies based on present requirements, that ANP is not useful. A recent paper of the Joint Chiefs of Staff, dated 19 July 1959, solidly supports this view, and stated that while no definite military requirement can be stated at this time, the continued development of ANP is considered as very important and potentially very useful.

> It is our view that during most of the last 13 years and the expenditure of most of the $900 million, the ANP program has been characterized by attempts to find short cuts to early flight and by brute force and expensive approaches to the problem. Thus we find that only a relatively very small fraction of the funds and energies applied to this program has gone into trying to develop a reactor with a potentially high performance. Most of the resources have been applied to attempts to develop materials which could "fly soonest"; to develop turbine machinery; to build facilities, many of which would only be needed in support of a flight program; to conduct experiments on the radiation resistance of tires, oils, insulation, electronic components, etc; and to develop new components for use in the unique environment which would be encountered only in the divided-shield situation as found in CAMAL and the old WS-125A. As a result of this approach to the problem we are still at least four years away from achieving flight with a reactor-engine combination ... which can just barely fly.

It is, of course, fruitless now to speculate on whether a statement of faith in the ultimate utility of ANP instead of the weapon-system argument would have won approval for the large appropriations needed to develop this engine. This kind of justification would probably have reduced the pressure for an early operational date and might have led to focusing the initial effort on reactor development. But even with the weapon-system objective, in view of the complete absence of prior development experience on a high-temperature reactor suitable for a flight system, the investment risk should have been limited by holding in abeyance investment in other engine components until an indication of interesting performance was obtained on the reactor. The turbojet-engine-component developments contribute little to the legacy of advanced technology derived from this program, and their elimination, along with other ancillary items, would have saved nearly half of the total investment. Furthermore, because the reactor was the long-lead-time component, some delay in starting on the other parts of the engine would not necessarily have delayed the engine operational date.

Where does ANP stand now? A case for an application based on cost effectiveness is still difficult to make. When chemically fueled

engines can perform the same mission, they usually prove superior to the nuclear system in terms of cost effectiveness except for some very special cases of uncertain military importance. The nuclear system must find justification in the exploitation of its unique extreme-endurance capability. Possibly experience with nuclear-aircraft operations may lead to the invention of an important application made feasible by the aircraft's flight endurance. Or, as in the case of the nuclear submarine, the invention of a weapon may suddenly give this system new importance. At present it would require an arbitrary high-level decision, probably based largely on faith, to reinstate the development of a nuclear airplane for investigating long-endurance applications. In the absence of this decision, a periodic re-evaluation of the performance potential provided by growing reactor technology and a research program to upgrade this technology are at least justifiable.

Appendix A

ILLUSTRATIVE ROCKET DEVELOPMENT TESTS

This appendix contains several tables taken from the developmental test programs for two rocket engines. These tests represent a small part of the total program.

Table 5

SAMPLE PROBLEM SUMMARY IN THE DEVELOPMENT OF THE TURBOPUMP FOR ROCKET ENGINE A

Problem	Action
Axial thrust control unsatisfactory	Provide stronger bearings with increased diameter front wear ring
Turbine-manifold inlet guide vanes cracking	Provide thicker vanes Procure vaneless manifold with local increase in torus wall thickness
Turbine-manifold diaphragm-to-torus weld joint cracking	Procure and test diaphragms with Inconel buffer ring
Turbine-manifold hanger brackets cracking	Provide Hastelloy buffer strips
Oxidizer-seal carbon nose breakage and leakage	Shrink on ring
Oxidizer-seal snap-ring retention unsatisfactory	Lock retaining ring in housing
Shaft failure during oxidizer pump explosions	Redesign inducer-to-shaft and impeller-to-shaft attachments
General oxidizer-seal leakage	Provide dirt trap
Turbine stator vanes cracking	Grind leading edge to increase radius
Turbine-wheel failures	Procure and test thicker wheels
Fuel pump not meeting NPSH requirement	Redesign and test model

Table 6

SOME SAMPLE TESTS FROM THE DEVELOPMENT PROGRAM FOR ROCKET ENGINE A

Test Number	Primary Test Objectives	Test Results
1	Evaluate engine operation with the ignition monitor valve plumbed upstream of the hypergol sequence valve; effect of oxidizer-dome purge on cutoff characteristics; effect of gas-generator valve-warmant fuel flow during liquid-oxygen chill period	Cut off by redline-chart observer due to an erroneous chart setting; primary test objectives were satisfactorily attained
2	Check out engine and instrumentation, obtain turbopump operation data, and make vibration survey of turbopump and gas generator	Cut off by erratic operation of turbine-overspeed cutoff system; primary test objectives were satisfactorily attained
3	Evaluate main-oxidizer-valve opening and closing characteristics with insulated control lines; effect of oxidizer-dome purge on cutoff characteristics; effect of gas-generator valve-warmant fuel flow during liquid-oxygen chill period	Cut off by redline-chart observer when turbine inlet temperature erroneously exceeded redline; primary test objectives were satisfactorily attained
4	Evaluate fuel-rich shutdown characteristics, turbopump and gas-generator vibration characteristics, and effect of gas-generator valve-warmant fuel flow during liquid-oxygen chill period	Cut off by erratic operation of turbine-overspeed cutoff system; primary test objectives were satisfactorily attained
5	Evaluate main-oxidizer-valve opening and closing characteristics with insulated control lines; effect of oxidizer-dome purge on cutoff characteristics, and effect of fuel-injector purge on cutoff afterfire	Cut off by observer due to failure of fuel-pump balance-cavity pressure to read redline for this test; primary test objectives were satisfactorily attained
6	Evaluate operation of auxiliary-fuel pilot-valve operation (to be used for engine shutdown with decaying fuel-pump pressure), turbopump and gas-generator vibration characteristics, main-liquid-oxygen-valve control characteristics with increased open-close cavity bleed orifices	Cut off by erratic operation of turbine-overspeed cutoff system
7	Evaluate effect of oxidizer-dome purge on cutoff characteristics; effect of gas-generator valve-warmant fuel flow during liquid-oxygen chilldown period; turbopump and gas-generator vibration characteristics	Cut off due to inadequate thrust-chamber prefill; No. 1 main-fuel-valve housing failed and thrust chamber was cracked

Table 7

SOME SAMPLE TESTS FROM THE DEVELOPMENT PROGRAM FOR ROCKET ENGINE E

Test Number	Objective	Results	Planned Action
1	Trim	Flowmeter data bad	Change flowmeters and repeat run with longer run time for endurance
2	Trim repeatability	Flowmeter data questionable	Change flowmeters and repeat with longer run time for endurance
3	Trim repeatability	....	Set controls for better trim and run power-margin check
4	Power-margin checks with cool-down valves changed	Engine low on power	Modify injector and repeat power-margin check
5	Power-margin check with increased bore diameter of outer row of oxidizer spuds of injector	Low in power, no change from Run 4	Conduct run for thrust-calibration data
6	Thrust calibration	Three-point thrust calibration with engine running	Conduct first run of stall investigation
7	Stall investigations with installed check valve in chamber-pressure sense line to control to trap overshoot pressure and hold control open	Check valve in chamber-pressure sense line failed to hold control open. Check valve did not work as planned due to low pressure differential created by the P/R system on the control	Conduct power-margin check after injector change
8	Power-margin check with check valve removed from control system	Run aborted by burnwire abort; engine did light and burn the wire, but 0.01 sec late	Repeat run after checking out abort system

Table 7 (contd.)

Test Number	Objective	Results	Planned Action
9	Power-margin check	Run aborted due to high fuel-flow indication, bad flow-meter reading	Change flowmeter if required and repeat run
10	Power-margin check	Power-margin data questionable due to flowmeter data	Run full investigation on flowmeter and repeat run
11	Power-margin check	Engine out of trim, could not reach desired mixture ratio for power check	Adjust trim on engine, repeat run
12	Power-margin check	Power-margin check completed. $P_c = 310$ psia at $R_m = 4.5$	Install check valve in thrust-control chamber-pressure sense line and resume stall-margin investigation program
13	Power check and mixture-ratio excursion to check out thrust control for production engine after rework to install special gaskets in thrust control	Run was successful and the thrust-control operation was satisfactory over the mixture-ratio operating range	Install development control with tight fit on piston rings for comparison with previous run which had loose fit on piston rings
14	Power check and mixture-ratio excursion to check out thrust-control operation with tight piston-ring fit	Run was successful and the thrust-control operation was satisfactory	Install new injector for determination of the effect of geometry of the outer row of oxidizer spuds on engine power
15	Power check and mixture-ratio excursion to determine engine power level with this injector	Run aborted when remote trimmer on mixture-ratio valve did not operate	Repeat Run 14 after repairing broken wire in test-stand remote trimmer

Table 7 (contd.)

Test Number	Objective	Results	Planned Action
16	Same as Run 15	Engine power level with the new injector was lower than with the previous injector. Thrust-control operation was satisfactory and downward drift of chamber pressure was much less than on pneumatic reset control without stainless-steel reference spring	Install original thrust control and make a power check and trim run to check out after rework to install AF40 gaskets
17	Power check and mixture-ratio excursion to determine engine power with the new injector and operation of the thrust control with new type piston and no piston rings	Run aborted because chamber pressure was above abort limits. Posttest results revealed oxidizer-injector manifold pressure was patched into strip	Correct the erroneous patch in the ADR system and repeat run
18	Trim-repeatability run and check on the operation of the thrust control with new type piston and no piston rings	Repeatability was satisfactory and the thrust-control operation was satisfactory except for an abnormal undershoot after the start transient caused by not blocking the maximum bypass area of the thrust control	Make a cold-chamber start to check out the operation of the thrust control
19	Check out the start characteristics of the thrust control during a cold-chamber start	Start transient was satisfactory and run was successful. Indications during run and posttest inspection revealed water in igniter-chamber pressure tap	Make series of runs with reduced oxidizer-pump inlet pressure to determine the capability of the test stand for providing inlet pressure to satisfy the requirements of the production engines

Table 7 (contd.)

Test Number	Objective	Results	Planned Action
20	Check engine-start characteristics with low oxidizer-pump inlet pressure required for steady-state operation of production engines	Start was satisfactory and run was successful	Reduce oxidizer-pump inlet pressure 5 psia and repeat previous run
21	Same as Run 20	Start was satisfactory and run was successful	Repeat run
22	Power check and mixture-ratio excursion to check out thrust control for production engine after rework to install new gaskets and replace damaged bellows	Run was successful and thrust-control operation was satisfactory	Install development thrust control with stainless-steel reference ring
23	Make a run with nominal inlet conditions and no trimming to check thrust-control drift characteristics with the stainless-steel reference spring	Run was successful and downward drift of chamber pressure was significantly less than in pneumatic reset control without stainless-steel reference spring	Remove engine from test stand to allow production engine to be mounted for acceptance testing
24	Power check engine and trim. Determine response of double-lip seal, piston, actuated cooldown valves. Determine effect of low helium pressure	Power check of engine showed good repeatability	Check trim repeatability. Make increased-pressure helium-supply test
25	Check trim repeatability. Determine relative effect of increased helium pressure	Trim showed good repeatability. The relative effect of the high helium pressure accounted for an increase in shutdown impulse	Install new hydraulic pump and conduct a cold-jacket start

Table 7 (contd.)

Test Number	Objective	Results	Planned Action
26	Perform a power-margin check and trim the engine	Burnwire abort	Increase burnwire-abort time limit and repeat run
27	Perform a 120-sec trim check run	Run was aborted by chamber-pressure abort system	Increase chamber-pressure-abort time limit and repeat run
28	Perform check run	Engine start was normal but was shut down by advance program when chamber pressure drifted	Vent thrust control to vacuum and make power-margin and trim run

Appendix B

DEPARTMENT OF DEFENSE DIRECTIVE 3200.9 ON "INITIATION OF ENGINEERING AND OPERATIONAL SYSTEMS DEVELOPMENT"*

The following excerpts from this directive relate to the discussion in this Memorandum.

V. OBJECTIVES

A. The objective of Concept Formulation is to provide the technical, economic and military bases for a conditional decision to initiate Engineering Development.

B. The overall objective of Contract Definition is to determine whether the conditional decision to proceed with Engineering Development should be ratified. The ultimate goal of Contract Definition, where Engineering Development is to be performed by a contractor, is achievable performance specifications, backed by a firm fixed price or fully structured incentive proposal for Engineering Development. Included in this overall objective are subsidiary objectives to:

VI. POLICY

B. Application

1. All new (or major modifications of existing) Engineering Developments and Operational Systems Developments as defined in reference (b),** estimated to require total cumulative RDT&E financing in excess of 25 million dollars, or estimated to require a total production investment in excess of 100 million dollars, shall be in accordance with this Directive unless specific waivers are granted by written approval of the Director of Defense Research and Engineering.

2. Other projects may be required to be conducted in accordance with this Directive, in whole or in part, at the discretion of the DoD Component or as directed by the DDR&E.

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*This directive was issued on July 1, 1965, and supersedes the directive bearing the same number and entitled, "Project Definition Phase."

**DoD Instruction 3200.6, "Reporting of Research, Development and Engineering Program Information," June 7, 1962.

C. Concept Formulation

The experimental tests, engineering, and analytical studies that provide the technical, economic and military bases for a decision to develop the equipment or system will be accomplished in the Concept Formulation period. Conditional approval to proceed with an Engineering Development will depend on evidence that the Concept Formulation has accomplished the following prerequisites:

1. Primarily engineering rather than experimental effort is required, and the technology needed is sufficiently in hand.
2. The mission and performance envelopes are defined.
3. The best technical approaches have been selected.
4. A thorough trade-off analysis has been made.
5. The cost effectiveness of the proposed item has been determined to be favorable in relationship to the cost effectiveness of competing items on a DoD-wide basis.
6. Cost and schedule estimates are credible and acceptable.

D. Technology Advancement

The key criterion in the degree of technology advancement permitted in Engineering Development is the level of confidence in the probability of successful development. It is not intended that a system will be limited to an assembly of off-the-shelf components. It is intended that the technology that is required to meet a system specification not exceed in quantitative performance that which can be demonstrated either in development form or in laboratory form. Projection into Engineering Development of anticipated developmental achievement will be permitted only when sufficient quantitative results have been obtained, in laboratory or experimental devices, to allow such projection with a high confidence. In general, these projections will assume the probability of Engineering Developments matching but not exceeding laboratory results.

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10. ABSTRACT A discussion of techniques for handling R&D funds to reduce the investment risk in implementing programs for new propulsion systems, particularly systems which are novel and advanced. To guide the funding management, relevant R&D activities are described in detail, followed by a discussion of criteria to be met before an engine qualifies for consideration.	11. KEY WORDS R&D Cost analysis Propulsion Management